sented by one carrying positive electricity down or negative electricity up, whereas that at the North Magnetic Pole might be one carrying positive electricity up or negative electricity down. We say *probably*, because it is exceedingly difficult to imagine that either of these vertical currents goes through the lower regions of the atmosphere into the earth, and it is likewise very difficult to imagine that the system of currents is an open one. They must, therefore, somehow close themselves in the upper atmospheric regions, and we may thus perhaps imagine that, while we have an ascending current at the North Magnetic Pole, we have a series of descending positive currents at the equator.

Or, if we prefer to render the analogy between the meteorological and magnetical systems more verbally complete, we should say ascending negative currents at the equator and descending negative currents at the pole.

These vertical currents being supposed to be confined to the upper regions of the atmosphere, we might imagine that they ought to render themselves visible at the magnetic pole, where they are most concentrated. If so, they would appear as a luminous vertical curtain or fringe suspended in mid-air. This at once suggests to us that the well-known form and nearly continuous appearance of the aurora in these regions may be due to this cause, and may represent to us the vertical component of those currents which we have here supposed to be the causes of the solar diurnal magnetic variations. It must not, however, be supposed that in making this suggestion we imply that phenomena of an auroral nature are not likewise connected with magnetic disturbances.

It is to be remarked in conclusion that a system of atmospheric currents will act inductively on the terrestrial magnetic system, so that the final effect on the needle will be the conjoint effect of the currents above and of the magnetic change below. In the case of the declination it is our inability to express the force that acts near the equator or near the magnetic pole in terms of any conceivable general change in the magnetic system that induces us to look to atmospheric currents as affording us a simpler mode of expressing observed facts. This, however, does not hold for the horizontal force near the equator. A set of currents moving east in both hemispheres will produce by induction a definite and well-understood effect upon the terrestrial magnetic system. We do not, therefore, know how far the change produced by the sun upon this element is due to a cause above the needle or how much to magnetic change below; and in this respect the conclusions we have deduced may require modification.

ON THE DIURNAL PERIOD OF TERRESTRIAL MAGNETISM [1]

THE explanation of the daily variation of the magnetic forces observed on the surface of the earth will, in all probability, lead to the explanation of the mysterious connection between solar phenomena and terrestrial magnetism. For the increase in amplitude of the diurnal variation of the horizontal components of magnetic force forms one of the most striking effects accompanying the increase in sunspot activity. The daily variation, then, seems a most important symptom of solar influence, and its investigation becomes a matter of great interest.

In the remarks which I wrote out for the Report of the Committee appointed by the British Association for the purpose of considering the best means of comparing and reducing magnetic observations, I pointed out the importance of adopting a suggestion, made already by Gauss, to apply the analysis of surface harmonics to the diurnal oscillations. It is well known that such an analysis would allow us to decide the question whether the immediate cause of the disturbance was inside or outside the surface of the earth; nor can there be two opinions as to the importance of definitely settling that question. At the time I wrote out my suggestions, however, it seemed to me that, as the causes of the disturbance had their seat in all probability close to the surface, whether outside or inside, that we should require a large number of terms in the expansion before we could arrive at a definite result.

In this I was mistaken, and it is one of the principal objects of this paper to show that the periodic variations adapt themselves with great facility to the analysis, and that even with the very limited quantity of material at our disposal we shall be able to arrive at most important results; results which within a short time might be made absolutely certain if additional observations at a few well-selected stations are taken. My results, as far as they go, point definitely to the region *outside the surface of the earth* as the locality of the periodic cause of the variation. It is easy to see that, if electric currents parallel to the earth's surface produce any disturbance, we can readily find out whether these currents are outside or inside the earth. As we pass through any current-sheet, the normal magnetic force remains continuous, but that tangential component which is at right angles to the current suffers a discontinuity depending on the intensity of the current. For a spherical current-sheet these components will always be of opposite sign. If we then find the distribution of magnetic potential on the surface of the earth from the horizontal components only, we should get by calculation a vertical component of different sign according as the cause is inside or outside. A comparison with the observed values will at once decide the question. A more careful analysis is necessary, if the causes are partly outside and partly inside, and we wish to determine their relative importance.

I believe that few practical magneticians at the present day read Gauss's memoir "On the General Theory of Terrestrial Magnetism," and the loss which cosmical physics has suffered in consequence is, as far as our generation is concerned, quite irretrievable. The memoir is a model of scientific reasoning, and full of suggestions which are as valuable now as they were fifty years ago. The investigations of Gauss are founded on the assumption of a magnetic potential on the surface of the earth, but that assumption requires justification in the case of magnetic disturbances. There will be no potential if there is a discharge of electricity through the earth's surface, and a variation of electric charge would be equivalent to a current. Calculation shows that electrostatic experiments on the surface of the earth would have shown before now if there was a sufficiently rapid change in electric potential to cause a disturbance of the magnetic needle. As regards an actual discharge, it is difficult to form an estimate, and we have therefore to fall back on magnetical observations, and see whether or not they seem to show that the line-integral of magnetic force taken round a closed curve vanishes. The calculations of the author, made on the assumption that it does vanish, seem to show a general agreement with fact; but some observations of Sabine, taken near the magnetic pole, would, if confirmed, point to a discharge in the Arctic regions.

The determinations of the diurnal variation of the magnetic variations show such a remarkable regularity everywhere except in the Arctic regions, and especially in latitudes between 20° and 60°, that we may as a first approximation express the westerly force (measured as change in declination) as the product of two quantities, one changing with local time, the other with latitude only. This assumption leads to the conclusion that the northerly component of force ought to be a maximum or minimum when the declination-needle passes through its mean position. This is very nearly true at Greenwich, Bombay, Lisbon, and Hobart. The agreement is not quite so good at the Cape of Good Hope and in St. Helena, but the observations at these places show some marked anomalies. It is found by observation that the variation in declination increases with the latitude, and we may as a first approximation put it proportional to the sine of the latitude. Writing γ for the westerly, χ for the northerly component of force, u for the co-latitude, λ for the longitude reckoned towards the east, and t for the local time, we may put

$$\gamma = \cos u \cos (t + \lambda).$$

It follows from this, on the assumption of the existence of a potential, that

$$\chi = \cos 2u \sin (t + \lambda).$$

The important point here is the factor $\cos 2u$, which changes sign at a latitude of 45°. If our equation is approximately right, the northerly force ought to be a maximum in the morning, a minimum in the afternoon in the equatorial regions where $\cos 2u$ is negative, while in latitudes above 45° the minimum ought to take place in the morning. This is exactly what happens, with the exception that the change seems to take place in latitudes smaller than 45°. At Bombay the *maximum* of horizontal force takes place at 11 o'clock a.m. At Greenwich the *minimum* takes place a little after that time.

At Lisbon ($u = 51°$) the phase agrees in summer with Greenwich, and in winter with Bombay, the Greenwich type pre-

[1] Abstract of a Paper read before the Manchester Literary and Philosophical Society, by Arthur Schuster, F.R.S.

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ponderating; we may conclude that the latitude at which the change of phase takes place shifts with the season, and that its average position is not far south of Lisbon.

The good agreement of our formulæ with the observed facts encouraged me to deduce the vertical component of force. Measured downwards it should be

$$\sin 2u \sin (t + \lambda)$$

if the cause of the disturbance is outside the earth, but

$$-\tfrac{3}{2} \sin 2u \sin (t + \lambda)$$

if the cause of the disturbance is inside the earth.

Both expressions have their maxima and minima coincident with those for the northerly components of horizontal force, a fact which finds its confirmation in actual observation. They also show the phase of the vertical force to be the same for each hemisphere and not to change as with the horizontal force. But there is an important distinction: while the vertical force has its maxima and minima coincident with the maxima and minima of horizontal force at latitudes greater than 45°, in the equatorial regions the maximum of horizontal force ought to be coincident with the minimum of vertical force, and *vice versâ*, if the cause is outside the earth's surface; the opposite should be the case if the cause is inside.

At Greenwich the maximum of northerly force takes place at 7 p.m., the minimum at noon; the maximum of vertical force takes place at 7 p.m., the minimum at 11 a.m.

At Bombay the maximum of northerly force takes place at 11 a.m., the minimum at 9 p.m.; there is a very decided minimum of vertical force at 11 a.m; but there is no pronounced maximum; two minor maxima occur, one at 6 a.m. and the other at midnight.

As far as these results go they give an emphatic answer in favour of the supposition that a great part at any rate of the disturbing currents lie outside the earth's surface, a view which Prof. Balfour Stewart has often supported in the last few years. The results seem to me very encouraging, and I hope soon to be able to make use of more material and to obtain more accurate expressions for the various forces concerned.

If we make use of the actual observations of Bombay and Greenwich, we may calculate for each hour the intensity and direction of the currents which would produce the disturbance. This has been done, and the results have been collected in a table.

It is very remarkable how very nearly at the same local hours the currents flow north and south at Bombay and at Greenwich, namely, at 4 in the afternoon and between 7 and 8 in the morning. It is curious, moreover, to find how very quickly the current turns through the meridian: at Bombay, at 3 o'clock, it flows at an angle of 15° from the east, and at 5 already it flows due west, and remains almost unaltered in direction till 5 o'clock in the morning. At Greenwich the currents turn much less sharply, but they always flow east when the currents at Bombay flow west. The system of currents indicated is that approximately shown by the equations given in the paper, the phase, however, being different. Along the meridian on which the local time is 4, the currents flow from the equator towards the north; they turn round in our latitudes towards east and west, join on either side again to go south, where the local time is 7.30 in the morning, and come back along the equator.

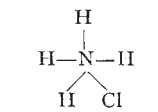
The strength of the currents is approximately of the same order of magnitude as the currents we are accustomed to send through our vacuum-tubes, but as the thickness of layer through which they are distributed must be very large compared to that on which we experiment, the current-intensity at each place is very small, far too small to cause luminosity. The currents, on the whole, are weaker at Greenwich than at Bombay, but, while they almost vanish at one time at Bombay, making the ratio of the strongest to the weakest current equal to 73, that ratio is only 3½ at Greenwich. The minimum at Greenwich in the early morning is as pronounced as the afternoon minimum, but much less so at Bombay.

On the whole, the numbers, both as regards direction and intensity, show such a remarkable regularity that there is good hope of obtaining a good mathematical representation of their distribution.

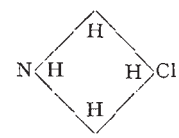
CHEMICAL AFFINITY AND SOLUTION

IN 1878 I read a paper to the Royal Society of Edinburgh, in which I stated my opinion, based on the results of a considerable number of experiments, that chemical combination solution and suspension of solids, such as clay, in water differ in degree only, and are manifestations of the same force; and that there seems to be a regular gradation of chemical attraction from that exhibited in the suspension of clay in water up to that exhibited in the attraction of sulphuric acid for water, which we call chemical affinity. Further, I stated that the attraction of chemical affinity is not, in all cases, at any rate, exhausted when a definite compound is formed, but has sufficient power left to form solution or suspension compounds. In 1881 I read another paper on chemical affinity and atomicity, in which I went a step further, and endeavoured to show that the theory of valency as usually held was incorrect in assuming chemical affinity to act in units or bonds, and insufficient to account for the various phenomena of varying atomicity, or valency, molecular compounds, crystallisation, solution, alloys, &c., and that all these varied phenomena were simply due to the chemical affinity of the elementary atoms, and that the difficulties disappeared if we got rid of the idea of the indivisible units of chemical affinity, and considered it as a whole acting all round, and spreading out, so to speak.

As an illustration of my views, I considered the compounds, HCl, NH_3, and NH_4Cl. In HCl we have two monovalent elements combined, and their chemical affinities completely neutralised or satisfied. In NH_3 we have N considered as a trivalent element satisfied with three monovalent elements. Now these two completed, neutralised or satisfied compounds combine with one another to form the third compound, NH_4Cl. How is this? The usual answer is that N sometimes acts as a pentavalent atom, and in this particular case does so, and the compound is represented graphically thus:



I pointed out that this explanation was most unreasonable, and to me, indeed, incredible, because it supposes that N, which has usually such a weak affinity for Cl, can nevertheless decompose the HCl into its constituent atoms, and fix the atom of Cl to itself. While on the other hand the Cl leaves the H, for which usually its affinity is so great, and unites itself to the N, for which usually its affinity is so small, and this while the atoms are in such close proximity, as they must be in a molecule, and with so many hydrogen atoms allied with it. My explanation was simply this. The affinity of the Cl acts on all the four atoms of H, and the affinity of the N does the same; and thus the whole molecule is held together, and may be represented thus:



I did not, however, exclude the idea that the Cl and N react on one another to some extent, but the *main* cause of the combination is as stated above.

Since these papers were read chemists seem to me to be coming more and more to my views in this matter. Thus Pattison Muir in his "Chemical Principles," says: "It seems to me that a most important step will be made if the bond theory of valency is generally abandoned; with it will go all those quasi-dynamical expressions, the offspring of loose and slipshod ways of thinking which have gathered round that strange anomaly, a unit of affinity employed as a variable standard for measuring nothing." Further, he says, in reference to the behaviour of acetic acid vapour when exposed to a high temperature: "If this is so, we evidently have a series of substances beginning with solution of salts or gases in water and proceeding through crystallisation and acetic acid vapour at low temperature, which connects mechanical mixtures on the one hand with stable gaseous compounds on the other."

Again, Professor Armstrong, in his address to the Chemical Section of the British Association at Aberdeen last year, says that in his view molecular compounds are held together by what, for want of a better name, he calls surplus, or residual affinity. In view of these and many other similar expressions of opinion, all tending in the same direction, I may perhaps be excused for again bringing forward the subject.

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